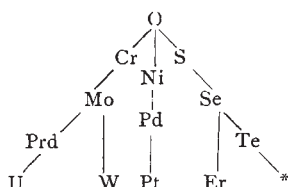


The system works out in such a way that each element in the first row of the periodic table becomes the parent of all the elements in its own vertical series.

Oxygen, for instance, is the root of the following genealogical tree:—



Chromium, nickel, and sulphur, in this way belong to the second generation. Molybdenum, palladium, and selenium to the third generation, and so on. The constants of the elements, such as atomic weights, densities, atomic volumes, specific heats, atomic heats, and their electrical and magnetic properties, their valency, &c., are then discussed with the view of justifying the mode of treatment adopted. It is here shown that on arranging the elements according to the author's system, besides the well-known relations between properties and atomic weights, additional simple numerical relations are traceable between the magnitudes of the atomic constants themselves, and also between these magnitudes and the numbers denoting the degree of condensation of the groups to which the elements belong. The use to which these may be put as a means of controlling the values of atomic weights and predicting the properties of undiscovered elements is indicated.

The second and not the least useful part of the book contains a collection of physical constants, from which the data used in the first part were chosen.

The book is a suggestive contribution to the literature on a subject which since the time of Prout has been prolific of speculation, but which even yet seems slow to condense and take a form sufficiently definite to warrant its being raised to the rank of a theory. J. W. R.

The Future of British Agriculture. By Prof. Sheldon. (London: W. H. Allen and Co., Ltd., 1893.)

THE opening chapters of this little book are devoted to the solution of the questions, "Will wheat-raising pay in Great Britain?" and "Is wheat to be no longer king?" After indicating the reasons which led to the enormous reduction of land under wheat—a decrease of something like 42 per cent. within the last twenty-five years—Prof. Sheldon comes to the conclusion, that, notwithstanding the importation of foreign wheat, and the fact that an ever-increasing demand for milk (of all farm products the least suitable for importation) necessitates larger areas of grass land, wheat-growing will not only continue, but may soon reach its former position, an event which he would not consider to be "a sign of unadulterated good." In connection with the question of wheat-production in the United States, there is one statement, made on the authority of leading American statistical experts, which we venture to think requires qualification, namely, "that in less than twenty years from 10 to 15 per cent. of the people's food will have to be imported into the United States." This is a point on which there may well be diversity of opinion, but, as pointed out by Messrs. Lawes and Gilbert in their recent paper on "Allotments and Small Holdings," the conditions will be quite changed with increased population, rotation will gradually become general, yielding various food products for home consumption; the soil will be better cultivated, yielding much larger crops of wheat where it is grown; straw and manure will no longer be burnt or wasted; and, lastly, there are still considerable areas of rich prairie land to be brought under the plough. So that it is probable that increased density of population will less rapidly diminish the

capability of production for export than may, at first sight, be supposed.

Perhaps the most interesting chapters are those on dairy farming; and it will afford a good deal of consolation to the dairy farmers of this country to learn that Prof. Sheldon believes "the competition of the United States is within measurable distance of its limit."

The book concludes with a chapter on a most important subject—tenant farmers' interests. The author states his view of the matter in his usual clear and forcible manner, and incidentally refers to what he terms "exploded, impossible 'Protection,'" and to "that new economic craze, 'Bimetallism'."

We welcome the book as a valuable contribution to our agricultural literature, and as a useful guide to those branches in which the author is especially qualified to instruct.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

Mr. H. O. Forbes's Discoveries in the Chatham Islands.

I WRITE a final line on this subject to express my regret that I should have misunderstood Prof. Newton and attributed to him (NATURE, p. 126 above) opinions in regard to the relationship between *Erythromachus* and *Aphanapteryx* which he does not hold.

On a point of accuracy, however, in regard to the "slight confusion of dates," allow me to say that I am sure no one will admit more readily than he that this had occurred, when I remind him of his letter to me of December 22, 1892 (now before me), in reply to a note of mine requesting him to be so good as to repeat his suggestion in regard to the name for the new genus, which I was about to describe, as I had mislaid his former note. "I have no memorandum," he says, "of what I suggested to you, but only an indistinct recollection that it was *Diaphorapteryx* . . . or something like that." This was, therefore, the date of the re-suggestion, and not my visit to Cambridge on February 23, 1893. *Diaphorapteryx* was described as a new genus in the Bull. Brit. Ornith. Cl., December 31, 1892.

HENRY O. FORBES.

The Fundamental Axioms of Dynamics.

A VERY brief reply to such of your correspondents as have favoured my paper with direct or indirect criticisms will at the present stage of the discussion be sufficient.

Referring first to Prof. Rücker's letter on p. 126, I acquiesce in the greater part of it—especially in its concluding paragraph, but it may clarify matters if I explain (1) that I do not contemplate parts of the ether, but regard it as an absolute continuum. Not the slightest advantage is gained by pushing action and distance back a step or two—it must be exterminated. (2) That I have no faith in "action at constant distance" other than distance zero. The reason such a phrase ever appeared in my papers is because that is all I am able to deduce from the assumption of the conservation of energy. It requires identity of energy to prove absolute contact. Hence I prefer to work backwards, and, assuming universal contact action or the denial of action at a distance, to deduce therefrom both the conservation and identity of energy.

Prof. McGregor contradicts three statements in the Report of the meeting of the Physical Society (p. 117), a report which is usually admirably done, and which was well done in this case. Though not responsible I reply to his three points categorically:

(1) He was understood to object to the Newtonian statement of the first law—not to the fact or law itself.

(2) A reference to the first two pages of his paper in the February *Phil. Mag.* will show him, I think, that he has now partially forgotten what he said on the second head.

(3) It is to be admitted at once that the phrase "equally well," not "well," was employed.

Now, turning to Mr. Dixon's letter (p. 149), it does not seem to me that he is perfectly candid. He accuses me in his concluding paragraph of an unfair practice by omitting the word "direct," but no such word occurred in his letter on p. 103, to which I was replying; nor is what he now says consistent with the surface meaning and intention of the second paragraph in his former epistle, so far as I can judge. A withdrawal of that hasty and misleading paragraph is what I had expected from him.

In the first paragraph to his recent letter he explains why he considers that the fact that potential energy belongs to a system is hostile to the idea of identity, but his proof does not appear to me valid unless the phrases "belongs to" and "has no local habitation within" are considered identical. If he can show that a given portion of potential energy "has no local habitation within a system," he will undoubtedly be usefully attacking the proposition that it possesses identity, but I do not see that he has even attempted such a proof at present.

OLIVER LODGE.

Popular Botany.

I VISITED Tyne Dock yesterday, in order to attempt to solve the question put by Mr. A. W. Bennett in your issue of the 1st inst.

The plants which caused the fatality grow in a small hollow close to a newly-opened road. The surviving child is but five years old, and therefore much too young for any evidence of hers to be convincing.

There seems little doubt, however, that the hemlock, *Conium maculatum*, brought about the death of the other two children. Large quantities of this, looking very attractive just now, are growing on the spot, together with smaller quantities of *Heracleum sphondylium*, *Anthriscus sylvestris*, and a very few plants of *Bunium flexuosum* at the margin of the hollow. No other umbelliferous plant is growing near. Yesterday, troops of children were gathering the young and pretty leaves of the hemlock, and making them up into bouquets with grasses and flowers.

The children, who died from the effects of eating the plant were aged respectively four and five years, and probably, in common with thousands of others in the district, would not recognise cabbage if they saw it growing, which very likely they never did. I have met many very much older children here who are as ignorant of common garden and field plants.

Gateshead-on-Tyne, June 12.

JOHN BIDGOOD.

The Big and Little Monsoons of Ceylon.

IT is well known to all Anglo-Indians, even the least scientific, that the summer monsoon is ushered in by two periods of rain-burst, called respectively the chota and burra barsât. The former occurs sometimes in April or May, and the latter in June or July, the precise dates varying not only with the locality but with the year. The chota barsât only lasts a few days, and is looked upon as the advance guard of the burra barsât, or great rains.

The conditions which tend to produce the chota barsât have not, so far as I am aware, been studied in detail, but are probably similar in character though on a smaller scale, and more local than those which regulate the inception of the burst of the monsoon, as it is popularly termed. It can be readily understood that as soon as the solar rays are sufficiently powerful to heat up a portion of the land area, and by lowering the pressure to determine an rush of surrounding marine air, condensation and precipitation will occur much in the same way as in the burra barsât when the air over the whole peninsula has become heated, and the saturated air from the equatorial Indian Ocean rushes in in a large and continuous stream towards the low pressure area thus formed. In the former case the conditions are not only more local and ephemeral owing to the small amount of vapour formed over a comparatively cool sea, but are mixed up with the residue of the cold weather disturbances, which are due to anti-monsoon conditions.

Mr. Blanford, in his admirable monograph on the rainfall of India, has compared the direct solar action which sets the monsoon in action to the pull of the trigger, by which the intrinsic latent energy of the resulting air-stream is shot forth. In the case of the chota barsât the comparison holds equally good only the resulting charge is feebler.

Now it has been recently maintained that while the distribution of temperature anomalies in the Indian peninsula regulates the inception of the little monsoon and its accompanying chota barsât, it is only when the central Asian plateaux become warmed up so as to produce an inflow beyond the Himalayan barrier, which must consequently affect the upper as well as the lower atmospheric strata, that any general deep movement of the equatorial vapour-laden air occurs on a scale sufficient to produce general-monsoon rains. That in fact there are two movements, one in the lower air, and the other in the air above the first 5000 or 6000 feet, and that it is only when the two occur coincidentally that we get the grander phenomena which accompany the burst of the big monsoon, as they term it in Ceylon. Some such theory appears necessary to account, not merely for the peculiar suddenness of the burst, but also for its variable date of arrival in different years. Until, however, we know more of the meteorological conditions of Central Asia and Thibet, this hypothesis must remain in a tentative state. Meanwhile, however, it is undoubtedly valuable to find that these two periods of rain-burst are not only distinct enough to be referred to under separate names over a large part of India, but in Ceylon are considered so important as to have their dates separately recorded by the Marine Master attendant at Colombo. In the excellent Ceylon *Mercantile and Planting Directory*, edited by the late Mr. A. M. Ferguson, and now carried on by his successor, Mr. J. Ferguson, a list is given of the dates of commencement of the little and big monsoons, from 1853 down to 1892 inclusive.

As a general result it is found that the average dates for the little and big monsoons are April 20 and May 19, and that when nothing particularly abnormal occurs, the big monsoon may be expected to follow the little one in about a month.

There are, however, considerable variations from this normal, the little monsoon date ranging through 52 days, and the big monsoon from May 1 to June 19.

On looking over these variations it struck me that they would probably be found to correspond to some extent with the rainfall of adjacent localities in India, especially the Carnatic. The result of a comparison of the anomalies is shown below—

CEYLON. ¹				CARNATIC. ²	
Date of arrival of the big monsoon, before or after its average date, May 20.				Mean rainfall anomaly in inches (40 stations).	
+ Before - after				Inches.	
days.					
1864	...	+ 5	...	-	5
1865	...	- 11	...	-	5
1866	...	- 2	...	-	4
1867	...	- 30	...	-	9.4
1868	...	- 14	...	-	4.6
1869	...	- 28	...	-	0.3
1870	...	+ 9	...	+	1.8
1871	...	+ 7	...	+	5.5
1872	...	+ 18	...	+	11.5
1873	...	- 4	...	-	0.1
1874	...	+ 15	...	+	7.3
1875	...	- 8	...	-	5.2
1876	...	- 17	...	-	13.2
1877	...	+ 4	...	+	8.3
1878	...	+ 1	...	0	
1879	...	0	...	+	2.3
1880	...	+ 5	...	+	7.0
1881	...	- 9	...	-	2.1
1882	...	0	...	+	4.4
1883	...	+ 10	...	+	5.2
1884	...	+ 5	...	+	11.6
1885	...	- 16	...	-	1.1

A mere glance at these figures shows at once a remarkable parallelism both in signs and numbers. Thus in eighteen years the signs are alike, neutral in three, and unlike only once. As it is well known that the rainfall of the Carnatic was found by Mr. Blanford to vary in a cycle of eleven years, closely corresponding with that of the sunspots,³ the same ought to hold for the anomalies in the dates of arrival of the big monsoon at Colombo. As a matter of fact the relation appears to be still

¹ From the Ceylon Directory, 1892.

² From the Rainfall of India, Part II., Indian Meteorological Memoirs, 1887.

³ Mr. Blanford computed the probability of such a cycle as compared to an invariable average to be as 655:1. Indian Meteorological Memoirs, vol. iii. part 2, p. 244.